tain rather special aspects of scientific life; we are shown very little of the managerial and political roles that scientists play, very little about obtaining grant support, or planning new projects, or interacting with subordinates, or doing business at conferences. Having said that, I should make clear that Judson does not provide an idealized picture of science: some people's faults are plain to see, as are the virtues of others, and there is ample reference to intense competition, lasting enmity, biased appraisal of work from low-status laboratories, and collective error and confusion. This is post-*Double Helix* science writing.

The book then is a careful description of the formative years of molecular biology that makes fascinating reading. It is a book that contains a great deal of information; it offers a lot of clues to the historian, suggests lines to follow up, themes to develop, and models that could be tested against its data, but in general it does not operate on this second-order level itself. Judson does make one general historical claim, that the history of molecular biology can be regarded as the development of the concept of biological specificity, apprehended from the convergent points of view of genetics, biochemistry, microbiology, physical chemistry, and x-ray crystallography. This is an interesting idea, and it immediately raises questions about the nature of the process through which this concept was deepened or elaborated and given new levels of meaning. I can best explain this by discussing briefly three issues raised by Judson's book.

One of the recurring ideas in *The Eighth Day of Creation* is that of a style of reasoning specific to molecular biology, a style that requires one to make simplifying assumptions, to exercise a certain boldness of supposition, in advance or in defiance of the data, and to reason theoretically. At several points Francis Crick talks of the difficulties and satisfactions of pursuing this type of argument while evading the clutter and distraction of chemical detail. Brenner mentions a cult of minimal experimentation. Certain experiments are presented as models of elegance and intellectual parsimony, although on occasions the method broke down and led molecular biologists astray. Now such a style can be maintained only if it works, that is, if it gives results or models to test; and if it can be practiced, that is, if institutional conditions permit this innovation and the admission of this mode of argument into the domain of biology. This raises interesting questions about the development of molecular biology, about how the conceptual ground was prepared for such theoretical reasoning and how such unconventional styles of argument gained and sustained professional legitimacy.

This leads to a second issue, which one might call the paradox of information theory. Judson argues that the postwar information sciences (cybernetics, control theory, cryptography, the theory of programming, and others) played no direct role, with a few minor exceptions, in the formation of the concepts of molecular biology. In this he is persuasive, but the problem still remains of how the ubiquitous presence of terms like "code," "message," "feedback," "information," "reading head," and "program" within its contemporary discourse is to be explained. This system of terms is not a mere façon de parler; it is the outward manifestation of a set of deeply rooted rules of thought. How and why was such a transformation effected and which groups and individuals brought it about? Clearly we need to know more here.

The third issue is a more general one, concerning patterns of professional interaction in science, particularly as a new field develops. Judson offers us comments, capable of generalization, on the way the phage group was set up or the manner in which molecular biology was established at the Institut Pasteur and on the international network of communication and collaboration that developed. He also describes instances of noncommunication where one might have expected more interaction, and in the final chapter he discusses the coalescence of five approaches to establish this new science of life. I wish he had gone further here and worked at the creation of some organizing model, about how the new groups formed, how the changing approaches to theory and experiment were classified and an institutional identity established. This remains for others to do, making use of the fascinating material assembled in *The Eighth Day of Creation*.

EDWARD J. YOXEN
*Department of Liberal Studies in Science, University of Manchester, Manchester M13 9PL, England*

# SALT: Questions and Interpretations

**The SALT Experience.** THOMAS W. WOLFE. Ballinger (Harper and Row), Cambridge, Mass., 1979. xviii, 406 pp. $18.50.

**Arms Control and SALT II.** W. K. H. PANOFSKY. University of Washington Press, Seattle, 1979. viii, 76 pp. Cloth, $6.95; paper, $2.95. Jessie and John Danz Lecture Series.

The United States Senate is now debating the ratification of the treaty resulting from the second phase of the strategic arms limitation talks (SALT II) with the Soviet Union. One might assume that such agreements are of overwhelming importance, for they concern the missiles and bombers by which the United States and the Soviet Union threaten and deter attacks on each other, and perhaps keep peace in the world. In the face of worries that technology will make land-based missiles vulnerable to attack in the 1980's, or that weak American bargaining may have given the Soviets too many advantages since SALT I, one might have expected a great public interest in the issues discussed in the books under review.

Yet the level of public information on the SALT talks is remarkably low. In polls taken this year as few as one quarter of Americans could correctly identify the United States and the Soviet Union as the two parties to the negotiations, and fewer still could answer questions about the details of the proposed agreements. In light of the importance attached to the negotiations, both by those favoring the SALT II agreements and by those opposing them, how can one explain this apparent indifference?

One reason for indifference might simply be that we have reached such a level of mutual assured destruction, of mutually reliable second-strike strategic force capabilities, that development of additional weapons by either side makes no difference. The world, in this view, is not in danger of a Soviet sneak attack, or a World War III, regardless of whether SALT fails or succeeds, and any such danger is not significantly increased by new Soviet missile developments or by any Soviet hard bargaining about the terms of a treaty. If the public seems to care less about the details of the strategic arms balance today than it did in the days of Sputnik and the "missile gap," in this view, it is because the dangers are indeed less real today.

In another view of the situation the lack of public awareness would be seen either as representing foolish optimism

or as representing psychological repression of unpleasant reality. If the former, Americans are being lulled to sleep by the mistaken expectations of détente while the Soviet Union is acquiring missile forces (accompanied by augmented conventional military forces) that will one day allow it to take a very hard line during some crisis. The leadership of the United States is already being somewhat intimidated and our European allies "Finlandized," in this view. Whether the crisis be about Tito's succession, or access to Berlin, or the future of Rhodesia or South Korea, the enhanced strength of the Soviet Union for a hypothetical World War III will translate into a substantial intimidation of the United States and (as a result) of its allies. Perhaps, as suggested, most Americans have, as San Franciscans do with earthquakes, simply elected to avoid thinking about the dangers of the 1980's.

One thus sees the same phenomenon linked to two very different interpretations of reality. Americans who seem to be ill informed about SALT may stay that way because the situation is so secure that they feel no need to learn any details, or because it is so menacing that they are afraid to open the box. The signals are mixed. While some opinion polls show a strong endorsement for SALT II and for any major strategic arms limitation agreement between the Soviet Union and the United States, others show a greater support for expanded defense spending, as compared with the years immediately after the end of the Vietnam war.

Opponents of SALT II contend that many poll questions are unconsciously biased toward getting Americans to endorse a SALT agreement that cannot deliver what they expect it to. Organizations like the Committee on the Present Danger have offered polls of their own allegedly showing that most Americans are over-rating the specific accomplishments of the SALT II agreement and that Americans would indeed prefer that the United States be strengthened than that SALT II be ratified. The questions of this poll have also been accused of tendentiousness.

As a supplement, not necessarily an alternative, to either of these interpretations of public ignorance, a third kind of explanation can be offered, simply that the issues have become extraordinarily complicated with respect to both substance and procedure, amid limits and sublimits, treaties and protocols, and the like.

Can it indeed be that the processes of formal negotiation (in place of the tacit bargaining of the old days, which let each side make its procurement decisions in response to the other side, knowing that such decisions would be seen as a signal and spur to further decisions on the other side) have actually made things more complicated than they need to be? Can it even be that SALT, with its lawyerly proceedings, has increased rather than decreased the strategic arms race over the past ten years? Has it been a cover for a Soviet move into superiority, or simply a part of an inevitable loss of American superiority? To return to our earlier question, can superiority have any meaning anymore, can missile totals make any strategic or political difference?

The two books under review are of great value in helping the reader through the maze of such questions, sorting out the substantive and procedural details of SALT and the broader questions of impact. Thomas W. Wolfe's *The SALT Experience* provides an overview of the SALT negotiations from their inauguration in 1968 to the beginning of this year. Wolfe is expert on the Soviet Union, on the strategic arms balance, and on American policy-making processes, all of which are extensively discussed. The book is clearly written, giving detail but not letting the detail stand in the way of more macroscopic analysis. It builds on John Newhouse's *Cold Dawn* but is more complete and less wedded to telling the "inside story."

Wolfe does not preach or plead for specific solutions but rather provides a comprehensive survey of theories and interpretations of what the SALT process has amounted to. As it stands, his book probably is the fullest and most balanced overview of the process that one can find in print. Wolfe's conclusions about the net impact of SALT are appropriately mixed and tentative. SALT is neither a clear failure nor a clear success in slowing the arms race. In some respects it may have headed off new rounds of arms acquisitions, but in other respects the litigation and "bargaining-chip" mentality of the process may have expanded such acquisitions. The United States is not in clear danger of Soviet attack; yet the growth of Soviet strategic potential cannot simply be shrugged off.

*Arms Control and SALT II,* by Wolfgang Panofsky, is a short but very useful complement to the Wolfe book. Essentially the text of two lectures delivered at the University of Washington, the book gives the reader the details of SALT II as disclosed in the spring of 1979 but begins with a more general overview of what the U.S.-Soviet strategic arms issue has been all about. Panofsky clearly represents the mainstream of American analysis on arms control and nuclear strategy. His view is that one should not expect SALT to achieve major or total disarmament, for the peace we enjoy depends on mutual assured destruction, on the ability of either of the superpowers to devastate the cities and industry of the other no matter what the other has done first. Rather SALT should be seen as a way of heading off wasteful and irritating expenditures on additional and redundant weapons systems or of heading off new developments in missile warfare that could threaten the retaliatory capabilities of either side.

Panofsky thus supports the SALT II agreements against critics from both right and left. He dismisses the idea that the United States will face any meaningful Soviet superiority if the agreements are accepted, or that verification of Soviet compliance with the agreements would be too difficult. He also rejects the liberal criticism that SALT does too little for disarmament, echoing many of Wolfe's comments about the limits of what it is reasonable to expect of the SALT process.

With respect to the original question of this review, Panofsky rejects any assumption that nothing very worrisome could happen in this area anymore, or that SALT and new research and development in strategic weapons systems cannot make any difference one way or the other. A scientist himself, Panofsky is all too aware of how science can acquire a momentum of its own that could upset the strategic balance upon which world peace depends. If the SALT talks can check this momentum somewhat, that in itself would be a reason for all of us to take SALT more seriously, although Panofsky, like Wolfe, is somewhat pessimistic as to whether SALT can suffice to apply the necessary restraints.

If Americans do not seem to worry very much about SALT as the Senate debate moves along, it thus remains unclear how well grounded their lack of concern is. Given that the Russians clearly have acquired more missiles than many "doves" would have predicted in 1970, some kinds of worry at least need to be addressed. The fear of excessive spending on arms might be balanced against a fear that Americans might suddenly become concerned, and intimidated, in the future. The risks of an unconstricted technology similarly gen-

erate threats to strategic stability that would better be headed off. Readers who wish to be informed themselves, and to contemplate why many of their fellow citizens have chosen to be otherwise, will do well to read the Wolfe and Panofsky books.

GEORGE H. QUESTER
*Department of Government,*
*Cornell University,*
*Ithaca, New York 14853*

## Littoral Philosophy

**The Outer Shores**. From the Papers of EDWARD F. RICKETTS. Introduced and edited by Joel W. Hedgpeth. Mad River Press, Eureka, Calif., 1978. In two volumes. Part 1, Ed Ricketts and John Steinbeck Explore the Pacific Coast. xii, 128 pp., illus. Paper, $7.95. Part 2, Breaking Through. x, 182 pp., illus. Paper, $9.45. The two volumes, $15.97.

James Boswell was one of the greatest biographers who ever lived, but his talents would have been largely wasted had not his friend the great lexicographer provided him with something to write about. A rather similar relationship holds between John Steinbeck and Edward F. Ricketts.

Ricketts is the culture hero of marine biology. He founded the modern study of intertidal zonation, and wrote, with Jack Calvin, a classic book on seashore life, *Between Pacific Tides*. Steinbeck and he collaborated on the equally celebrated *Sea of Cortez*. To the general reader he is known as "Doc," the hero of Steinbeck's *Cannery Row* and *Sweet Thursday*.

Unfortunately, Steinbeck's writings have tended to provide a distorted impression of what Ricketts was like and what he did. Hedgpeth has gone a long way toward rectifying this situation by publishing a series of manuscripts and letters interspersed with much helpful and entertaining commentary.

The documents show that Ricketts was a serious marine ecologist, in many ways ahead of his time. This is particularly evident in his communications to Steinbeck intended to serve as a basis for their projected book on the Queen Charlotte Islands. Of equal or even greater interest to many readers will be the exposition of Ricketts's philosophy, a curious mixture of Taoism, University of Chicago vitalism, and other doctrines, but dimly understood by Steinbeck. The choice document here is an early version of the essay on "non-teleological thinking" which Steinbeck adapted as a chapter for *Sea of Cortez*. Ricketts was concerned with deep metaphysical issues, but it is what we would call his "philosophy of life" that has made the strongest impression on the public. Ricketts bears comparison with Thoreau in this respect.

Some might contend that Hedgpeth has allowed too much of his own personality to intrude into this work. Yet he too has become a semilegendary figure in marine biology, and he actively participated in the story he tells. Hedgpeth has rarely if ever minced a word, and he does not conceal his feelings about those in academia and the publishing trades who take it upon themselves to manage the truth. With a few apt remarks and choice quotations he makes the late W. K. Fisher of Stanford look like a complete fool for seeing to it that *Between Pacific Tides* was Bowdlerized. A letter from Steinbeck to the publisher of *Sea of Cortez* expresses, in the language of outraged indignation, a steadfast resistance to efforts to lie about who wrote that book. The account of how the book was reviewed should make anyone think twice before perpetrating the all too frequent vices of that genre.

One might suspect that Hedgpeth has had troubles of his own maneuvering this manuscript past the guardians of respectability. But once the Backbites and Sneerwells have had their say, the work will be welcomed by those who want to read about good science and good literature. We have here a delightful and most unusual book about two great men. Even Old Jinglebollix would want to own a copy.

MICHAEL T. GHISELIN
*University of California*
*Bodega Marine Laboratory,*
*Bodega Bay 94923*

Edward F. Ricketts in front of his Cannery Row shop, July 1936. [Photograph by Ralph Buchsbaum, from *The Outer Shores*, part 1]

## Hunter-Gatherers Today

**The Nutrition of Aborigines in Relation to the Ecosystem of Central Australia**. Papers from a symposium, Canberra, Oct. 1976. B. S. HETZEL and H. J. FRITH, Eds. Commonwealth Scientific and Industrial Research Organisation, Melbourne, 1978 (U.S. distributor, ISBS, Forest Grove, Ore.). x, 150 pp., illus. Paper, $5.95.

The Aborigines of Australia flourished, or at least survived, for more than 10,000 years in a difficult environment by hunting and gathering. At the time of European contact, the number of Aborigines is estimated to have been about 250,000. Early observers report a lean, fit, well-nourished population that maintained self-respect by ethnocentrism despite chronic hardships and occasional crises. The first century of contact was hard on the Aboriginal populations: conflict, disease, and out-marriage (of women, almost entirely) reduced the size of the Aboriginal group to 67,000 by the census of 1901. The Aboriginal experience is typical of 19th-century contact of hunter-gatherers with more "advanced" peoples. Extinction of the culture and absorption of the people into the dominant group (at the lowest social-class level) was predictable, and actually occurred in the case of the Tasmanians, some of the Khoisan groups of Africa, and many of the tribes of native peoples in North America. Since 1900, however, the typical process has changed to one of concentration into dense settlements, provision of welfare payments or charity to provide a scanty cash income, dependence on store-bought food, and eventually population growth from the continuation of relatively high fertility despite a level of disease higher than that prevalent in the dominant society. In Australia, by 1966, the census shows about 80,000 Aborigines living in towns and on rural "stations" parallel to the reservations of North America and the settlements found in Africa. Peterson reports that during the 1950's there were at